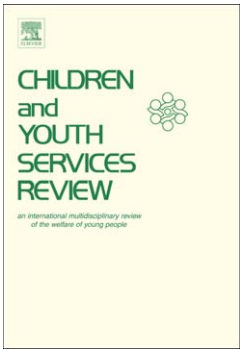

School enrolment following multisystemic treatment: A register-based examination among youth with severe behavioural problems

Marianne Dæhlen, Christian Madsen

PII: S0190-7409(16)30160-8
DOI: doi: 10.1016/j.childyouth.2016.05.016
Reference: CYSR 2976

To appear in: *Children and Youth Services Review*

Received date: 2 November 2015
Revised date: 24 May 2016
Accepted date: 24 May 2016

Please cite this article as: Dæhlen, M. & Madsen, C., School enrolment following multisystemic treatment: A register-based examination among youth with severe behavioural problems, *Children and Youth Services Review* (2016), doi: 10.1016/j.childyouth.2016.05.016

School enrolment following Multisystemic treatment: a register-based examination among youth with severe behavioural problems

Marianne Dæhlen, Norwegian Social Research (NOVA), Oslo and Akershus University College of Applied Sciences
Christian Madsen, Norwegian Institute of Public Health (NIPH)

Corresponding author: Marianne Dæhlen, e-mail: marianne.dahlen@nova.hioa.no

Abstract

This article addresses the ongoing debate about the effectiveness of multisystemic treatment (MST) by examining school enrolment at age 18 among youths who have received MST. The analyses are restricted to youths who engage in antisocial behaviour and/or substance abuse. We used propensity score matching to compare school enrolment between youths who had received MST and a control group who had not received MST. The analyses of population data showed a somewhat lower school enrolment in the MST group compared with youths receiving treatment as usual.

1. Introduction

Severe behavioural problems among youth are a matter of deep concern and are considered to be a major social welfare challenge (Olsson, 2010). In addition to the high rate of delinquency and substance abuse among these youth, research over several decades from several countries has shown high rates of school drop-out, unemployment, and adult criminal behaviour among delinquent and drug-abusing youth (see e.g. Marti, Stice, & Springer, 2010; Mensch & Kandel, 1988; Patterson, DeBaryshe, & Ramsey, 1989). In the 1970s, to reduce juvenile criminal activity and other types of disruptive behaviour, Scott Henggeler and colleagues at the Medical University of South Carolina introduced multisystemic treatment (MST).

MST is a short-term, family- and community-based therapeutic approach for families of youth aged 12–17 years with serious antisocial behaviour. Therapists are available 24 hours a day, seven days a week, and the treatment programme focuses explicitly on the family–school linkage (Brown, Henggeler, Schoenwald, Brondino, & Pickrel, 1999). MST is time-limited, with the average treatment period being three to five months (MST Services Inc., 2015). Initially, MST targeted youth with severe behavioural problems, such as delinquency, substance abuse and severe school problems. Currently, the target population has been expanded to other vulnerable youth, including abused and neglected youth, sex offenders and obese youth (for a review see van der Stouwe, Asscher, Stams, Dekovic, & van der Laan, 2014).

MST tries to achieve long-term results by keeping youth in their homes, in school, and out of trouble. In general, a key predictor of favourable long-term outcomes is education (De Ridder et al., 2012; Hammarström & Janlert, 2002; Rumberger & Lamb, 2003). In addition, reengagement in education has been found to help youth who received MST by giving them hope for the future and motivation to change their current behaviour (Tighe, Pistrang,

Casdagli, Baruch, & Butler, 2012). Thus, in this article we focus on school enrolment following MST. We ask whether youth who have been involved with child welfare services because of severe behavioural problems are still in school at the age of 18 because they have had MST.

This question has been answered affirmatively in the literature. Brown et al. (1999) showed that juvenile offenders who received MST improved their school enrolment compared with peers who received the usual services. Improved functioning for the MST group of juvenile offenders at school is also found in (Timmons-Mitchell, Bender, Kishna, & Mitchell, 2006). Furthermore, Henggeler et al. (1999) showed that youth with psychiatric crises who received MST instead of hospitalization were absent from school fewer days than those who were hospitalized. Moreover, Weiss et al. (2013) found a positive effect of MST on number of days present in school among adolescents with serious conduct problems. On the other hand, Barth et al. (2007) found that demographic background characteristics were more important in explaining educational progress than was MST. However, the Barth et al. (2007) study is based on a small sample and the authors urged caution in interpreting their results. Consequently, we do not know if MST increases school enrolment or if this positive relationship is because of a selection of the most resourceful youth into MST. Because there are only a few studies with somewhat ambiguous results, we need research on educational outcomes after MST.

In this article, we restricted our analyses to youth who previously received MST because of antisocial behaviour and/or substance abuse. The data for this study were drawn from Norwegian population data on child welfare clients. We examined school enrolment at the age of 18 among youth who engaged in antisocial behaviour and/or substance abuse and received MST, and compared them with similar youth who received treatment as usual (TAU). We used propensity score matching (PSM) to select youth sharing important background characteristics with the MST group for the comparison group (TAU). We conducted analyses exploring MST and school enrolment on 7,480 adolescents (MST = 1,086, TAU = 6,394).

The remainder of the article is organized as follows: we present an overview of the child welfare population and child welfare services in Norway; next we briefly review previous research on MST, followed by the methods and results of the current study; and finally, the article ends with a discussion of the empirical findings.

1.1 Child welfare clients and services in Norway

In Norway, almost four per cent of children younger than 18 years in a given year receive welfare benefits. The Norwegian child welfare system has a strong focus on assistance at home and family support; more than 80 per cent of all the children involved with child welfare services receive voluntary assistance in the home (Backe-Hansen, Madsen, Kristofersen, & Hvinden, 2014). There are more than 20 categories of in-home services, and the most frequent in-home intervention is advice and counselling. About one-third of child welfare clients receive advice and counselling (Christiansen, 2015). Manual-based parenting programs that target conduct problems (e.g., MST and Parent Management-Oregon (PMTO)) were first introduced in the late 1990s, and since then they have been implemented nationwide. Today, MST teams are available in all of Norway's 19 counties, though they are not available in some sparsely populated areas. The Norwegian Centre for Child Behavioural Development trains the 21 MST teams in Norway.

1.2 Past evidence of the effects of MST

Outcomes other than educational attainment following MST have been widely evaluated and several studies have shown that MST is effective in reducing delinquency and/or improving individual and family functioning (for an overivew see MST Services Inc., 2015). Positive outcomes following MST were also found in the only randomized controlled trials (RCT) conducted in Norway (Ogden & Hagen, 2006; Ogden, Hagen, & Andersen, 2007), which were based on a follow-up study to (T. Ogden & C. A. Halliday-Boykins, 2004). These studies compared the treatment group receiving MST with a comparison group receiving TAU.

However, the conclusion that MST leads to positive outcomes has been compromised by methodological difficulties, as Littell, Campbell, Green, and Toews (2005), Littell (2006), and Littell (2008) have argued. Littell and colleagues argue that MST offers no substantial benefits compared with the usual services and that the positive evaluations of MST are a result of methodological shortcomings and errors of interpretation in previous reviews. However, Henggeler, Schoenwald, Swenson, and Borduin (2006) have argued that Littell's analyses misinterpret and misrepresent MST research studies. The arguments that Littell makes on the one hand, and those that Henggeler and colleagues make on the other hand cover several areas, but one main dispute concerns methodological issues. Mainly, Littell argues that results from previous trials may be affected by unknown selection biases associated with drop-out and different levels of participation in MST. Henggeler and others do, however, disagree with this claim. Our aim in this article is not to address this

disagreement. Then again, the potential of bias due to attrition is minimal in registry data since we are able to identify the youths' educational attainment at the age of 18 independently of him/her taken part in the study. Individuals who died have been excluded from the analysis. In addition, any problems with selection are reduced by controlling for individual, parental, and geographical characteristics. However, it seems necessary to emphasise that the outcome measure following MST in the present article is limited to one single item – i.e. being in school or not at the age of 18 –, which is a more restricted measurement than previous studies. Consequently, the present study does not examine any effect of MST on unemployment, criminal behaviour, or other severe problems, In addition, any long-term effects on educational attainment is not examined.

1.3 Identifying selection and attrition biases

Most of the research on MST has been conducted as relative small, controlled trials using a so-called yoked design, which randomly assigns participants to receive either MST or the usual services (TAU) (e.g. Henggeler et al., 1999). Randomized control trials are often considered the gold standard for measuring the causal effect of an intervention. However, random allocation in trials is complex because allocation to the treatment group and the non-treatment group may differ not only with respect to treatment or not, but also with respect to other conditions that may have an impact on the effects of the intervention. For instance, MST is restricted to parents who are sufficiently involved with their children and motivated to start MST. Thus, it seems reasonable to assume that youth from the most disadvantaged families are excluded from MST, as Barth and colleagues have argued (Barth et al. 2007). Social stratification research has established that educational attainment is related to family resources, such as the parents' education, employment, income and/or immigrant background (see e.g. Blossfeld, Blossfeld, & Blossfeld, 2015; Jonsson & Rudolphi, 2011). As far as we know, previous studies have not adjusted for any impact of background characteristics on the effects of MST. In the present study, we include several background characteristics about the youths and their families, such as parental education, family income, the youth's gender and immigrant background.

In addition, whether or not someone is offered MST may differ by the characteristics of the community in which the youth/family live. Because MST is offered round-the-clock, it demands a relatively high number of skilled therapists, and some areas may not have enough trained MST therapists. This is particularly true in areas that are sparsely populated, as in many parts of Norway. With about five million inhabitants in an area somewhat larger than

Germany, which has about 80 million inhabitants, the population density in some parts of Norway is very low. Thus, MST is not offered in all parts of the country. Consequently, selection into MST and TAU may differ by characteristics of the youth, family and/or location. To address these issues, we included several indicators in our analyses to control for selection biases associated with the availability of MST.

In addition, in previous research on MST, many participants are lost to follow-up, although not in a pairwise fashion. Typically, the remaining participant of the MST/TAU pair is retained in the analysis when this happens. According to Littell (2006), this method poses a threat to the internal validity of such research. With regard to school enrolment following MST, it could introduce an invidious bias if MST youths with low school motivation are more likely to drop out of the trial. In this study, we have considered this by using information from public administrative registries, which resolves much of the problems of attrition (we do not need the consent of the youth, parents, or teachers to obtain such information).

Moreover, to the best of our knowledge, previous MST studies have been based on information collected from people involved in MST programmes (e.g., parents, teachers, and social workers). Consequently, evidence of positive outcomes for MST may have been artificially produced by collecting information from individuals with subjective perceptions of the MST programme (e.g. positive satisfaction bias, see Gail & Benichhou, 2000). The present study utilizes longitudinal register-based information. Consequently, this approach removes any biases in using self-reported measures. However, it should be noted that previous RCT-studies include information from several informants (youth, parent, teacher etc.), which reduces any problems with subjective perceptions. In the present study, the utilization of administrative records implies a lack of more complex outcomes, which rarely, if ever, are collected. At the present, only information about school enrolment at the age of 18 is available. Thus, it should be underscored that this study is limited to examining school enrolment at the age of 18 after MST.

The present study is guided by the following research questions:

Q1: Among youth, who have engaged in antisocial behaviour and/or substance abuse, are those who receive MST more likely to be in school at age 18 than those who receive TAU?

Q2: If youth who receive MST are more likely to be in school at age 18 than those who receive TAU, to what extent is this difference attributable to selection biases?

2. Data and methods

2.1 Study population

The data for this study came from the project Child Welfare in Norway 1990–2010, which is a large national longitudinal study to gather data about the characteristics and outcomes of children and families involved with the child welfare system. The data cover the period from 1990–2010 for 167,759 children and their families who have received child welfare services. Information about reasons for being in the child welfare system (e.g. behaviour problems/substance abuse), types of interventions received, years with interventions, age at first contact etc. is every year reported from the local government to Statistics Norway, and is assessed as highly reliable and valid data. For the present project, each individual was linked to other national registries (such as National Database of Education, Population data, Income data) through Statistics Norway using a unique personal identification number, which all Norwegian citizens have. We limited our study population to the birth cohorts 1990–1994 for two reasons: a) the longitudinal data do not include child welfare service data before 1990; and b) we have no available data on school enrolment at age of 18 after 2012. Of the children born from 1990–1994, 34,605 children/youths were registered with child welfare services during the period 2002–2010. MST was introduced in the child welfare statistics in Norway in 2002, and 6.4 per cent (n = 2,230) of the children in the 1990–1994 cohorts had received MST before the age of 18 at least once during the 2002–2010 period. Analysis (not shown here) showed that 57.6 per cent of all the youth who received MST were in school at the age of 18 compared with 73.7 per cent of youth who received TAU. Thus, before we restricted our analyses to youths with behaviour problems and/or substance abuse 16 per cent more of the TAU youth than the MST youth were in school at the age of 18.

Analyses show that out-of-home placement is more often provided to MST-youths than TAU-youths in the same age group (45.3 versus 25.1 per cent). To reduce bias, we also limited our study population to children/youths registered during 2002–2010 with behaviour problems and/or substance abuse problems with no out-of-home placement before or after receiving MST. We included the latter restriction based on the expectation that out-of-home placement is provided to youth with complex problems (more than 80 per cent receive in-home-initiatives in Norway) and that it would be difficult to take this complexity into account. With these limitations, the study population comprised 7,480 child welfare clients (MST = 1,086, TAU = 6,394).

2.2 Dependent variable

Compulsory primary and lower secondary schooling in Norway lasts for ten years and children start school the year they become six. At the age of 16, all youths have the right to free upper secondary schooling. This right comprises also youths with low school grades and/or youths who have skipped parts of compulsory school. About 97 per cent proceed directly from lower to upper secondary school (Statistics Norway, 2014) and starts on an academic or a vocational track. The academic track lasts three years, whereas the vocational track includes two years of classes and two years of apprenticeship. Consequently, students who do not drop out of school graduate at the age of 19 or 20, respectively.

We used the National Database of Education, Statistics Norway to determine whether participants were in school at the age of 18, which is a point in the youths' life where the MST/TAU was completed and at time where youth typically were in education. This variable was dummy coded, based on 1 = enrolled in an educational programme in the autumn of the year the participant became 18, and 0 = not enrolled in an educational programme in the autumn of the year the participant became 18. Information regarding type of education was not available, but this variable covers a wide range of educational programmes from basic education to education at the tertiary level. Usually, the youth were enrolled in educational programmes at the high school/upper secondary level.

2.3 Independent variables

Seventeen independent variables were used in this study to control for any selection differences by characteristics of the youth (age, gender, immigrant background, initial age for receiving child welfare services, average number of child welfare measures per year, experienced abuse), characteristics of the family (age, immigrant background (born outside the EU/EEA, USA, Canada, Australia or New Zealand), marital status, educational background, income, unemployment, receiving social welfare support, registered with substance abuse), and characteristics of the residential area (region of residence and population size).

The following variables were dummy coded: *gender*; *born in Norway*; *both parents with non-western background*; *registered with maltreatment*; *parents' marital status at the time of treatment*; *parental substance abuse before treatment starts*; *parental unemployment at the start of treatment*; *family received social welfare support*; and *population size*.

Three dummy variables were constructed for *parental educational level* - below upper secondary education, upper secondary education), and higher education.

Four dummy variables were constructed for *initial age for receiving child welfare services* (four years or younger, five thru nine years old, ten thru 14 years old, and 5 years or older). *Mother's and father's ages at the time child was born* were also coded by four dummy variables (19 years old or younger, 20 thru 29 years old, 30 thru 39 years old, and 40 years or older).

Parents' mean family income the last three years before treatment was divided into six income categories (see table 1). Five dummy variables were constructed for *residential region of Norway* (North, Mid, West, East, and South).

Average number of child welfare measures per year is the annual average number of welfare measures per child during the study period. The number of measures given ranged from 0 to 6. The variable was grouped into four categories, where the latter category (4) covers 4 or more.

Two variables had missing values: *Parents' average combined age at the time the child was born* (N = 28 missing), and *both parents with non-western background* (N = 271 missing). The median birth year for the fathers was 1963 and for the mothers it was 1966. The median parental background was "Norwegian". We used these median values to impute the missing values for parental age and parental region of origin, assuming that the data was missing completely at random (MCAR).

2.4 The propensity score matching

Randomized controlled trials (RCT) are considered the ideal design for causal inference, but is not free from biases. The external validity is threatened when the trial settings are not representative of the general population (Cook & Campbell, 1979). Likewise, the internal validity is threatened due to unavailable information of susceptibility or responsiveness to the treatment at baseline (Kravitz, Duan, & Braslow, 2004). Furthermore, RCTs are often not considered due to both ethical issues and high costs. Propensity score methods are considered as a nonexperimental option to RCTs. The propensity score is an individual's probability of being treated given his or her complete set of background information up until the time of treatment (Rosenbaum, 2002; Rosenbaum & Rubin, 1983). The essential point is the similarity of individuals and simplification of the analysis: equal probability is based on known observable characteristics, which reduce the analysis to one dimension (here: receiving or not receiving MST and the effect on the outcome). We included

the seventeen independent variables described previously and calculated the propensity score (or probability of MST given the seventeen variables) using a probit regression model (Pr ($Y = 1|X$) = ϕ ($X'\beta$). The aim is to evaluate the impact of MST on the population by calculating the average treatment effect on those treated (ATT).

This approach could in principle correct for bias given that all relevant variables were observed and measured without error. However, when a potential confounder to the treatment is unobserved, this approach can only correct for bias to the extent that the unobserved confounder(s) are correlated with the observed covariates (Luo, Gardiner, & Bradley, 2010). Thus, we cannot exclude the possibility that the treatment assignment is potentially entangled with the outcome due to unobserved characteristics than the ones that have been adjusted for (e.g. administrator introduced bias upon recruitment to MST, differences in IQ, school grades etc.).

There are several important underlying assumptions behind the PSM approach. First, conditional independence assumption (CIA) or selection on observables, assumes that the outcome is independent of treatment status after controlling for the observable covariates. This implies that, as far as we know, the assignment to treatment is random and allows the untreated participants to be used as counterfactuals for the treatment group. However, we can never rule out that an unmeasured covariate could have been a source of failure to the CIA. Since we cannot observe such a covariate, then the second best approach is to use a simulation-based sensitivity analysis as proposed by (Ichino, Meali, & Nannicini, 2008)

Second, the assumption of common support and the assumption of balancing property assume that there is sufficient overlap in the covariates used to balance the groups at baseline. This implies that the probability of receiving either treatment or non-treatment for each value of the vector is strictly within the region of each unit interval for comparable or balanced groups. It is recommended to restrict the sample to a group of treated and controls with common experience in order to reduce the amount of bias (Shadish, 2013). Several statistical packages have options for ensuring these assumptions in the analysis. Only when these theoretical assumptions are satisfied is it possible to claim that treatment assignment is strongly ignorable (Rosenbaum & Rubin, 1983).

Third, PSM assumes that the property between the treated and the controls are balanced. The estimated propensity score for each individual was used to match individuals using the “psmatch2” command in STATA (StataCorp, version 11.2). Several different approaches are available for PSM, and we tested to see if the results would be dependent on

type of matching strategy used. We also used the *pstest* module in STATA to evaluate the standardized differences in the unmatched and the matched sample.

In addition, we used *sensatt*, a STATA module, for simulation-based sensitivity analysis to derive point estimates of the ATT under different scenarios of deviation from the CIA (Nannicini, 2007). The simulation exercise gives us an indication to what extent the ATT estimate is robust to deviations from the CIA and is reported in the appendix.

3. Results

Of the 7,480 adolescents included in our analyses, 1,086 received MST during 2002–2010, and the rest (N = 6,394) received TAU in the same period. The latter group served as controls in our analysis. In the analyses and forthcoming tables, only adolescents with severe behavioural problems are included. Table 1 shows the frequency of adolescents registered as being in school at the age of 18 (dependent variable) for the MST and the TAU groups, with the descriptive statistics for the independent variables.

Table 1 about here

The results show that about six out of ten adolescents were in school at the age of 18. This share was somewhat lower in the MST group than in the TAU group (59.5 per cent and 62.9 per cent, respectively) and the difference is statistically significant. Thus, the results show that among youth with severe behaviour problems and/or substance abuse problems, the enrolment in school was somewhat lower in the group that received MST than in the group who received the usual services (TAU). This result does not support our initial assumption (Q1).

As also shown in table 1, the children that received MST are a highly selected group. When we compare them to their unmatched peers, boys born in Norway with higher educated western parents are more likely to receive MST. Furthermore, we see that higher (or stable income) increase the likelihood of receiving MST. The likelihood for receiving MST also increased if the family was situated in a large (more populated) municipality.

Figure 1 about here

The distribution of the propensity scores in the MST and TAU group is shown in figure. Overall, we find that there is good common support among the distribution of

propensity scores for both treated (MST) and controls (TAU). Only two in the MST group is off-support due to lack of comparable controls in the TAU group. After matching (table 2), these differences were close to zero. This suggests that our control is valid and that the balancing property is sufficiently satisfied in our analysis.

Table 2 about here

The next question concerns whether the somewhat lower share of MST youth enrolled in school at age 18 is related to differences between the MST and TAU groups in characteristics of the youth, their parents and/or their residential circumstances. In other words, is MST more likely to be offered to Norwegian youth with background characteristics associated with poor educational attainment rather than to youth whose characteristics are associated with successful attainment, as has been assumed (Q2)? Examination of the descriptive statistics does not support such an increased likelihood. The results in Table 1 (and Table 2) show that the MST group had relatively fewer boys, fewer youth with non-western immigrant backgrounds, fewer youth from families with low parental education and fewer youth with low family income. Previous research has shown that each of these characteristics is positively related to educational success (Jackson, 2013; Pettersen & Østby, 2013). In addition, the results show differences in the initial age for receiving child welfare services and residential characteristics, but differences between the MST and TAU groups on the remaining independent variables were small and non-significant. Small differences between the groups were also found for the number of initiatives received, with the TAU group having more initiatives at baseline (i.e. before receiving MST or TAU). Compared with the TAU group, the results show that the MST group has a higher share of youth with characteristics that have been found to have a positive influence on school achievement – i.e. in the MST group there are relative few boys, immigrants, and youth from families with low education/income. A follow-up question concerns whether the effect of MST on school enrolment is lower than indicated in Table 1, as the MST group is over-represented by characteristics that are related to positive school outcomes. Table 3 shows the estimated effect of MST and TAU on school enrolment by using different PSM strategies.

Table 3 about here

The results indicate that an overall smaller percentage of MST youth (59.5 per cent) than TAU youth (60.5 - 63.7 per cent) in school at the age of 18. Three of the models were statistical significant at the 0.05 level.

If school enrolment is related to differences in background characteristics for these groups of youth, we should expect an increase in the difference between the MST group and TAU group. The results in Table 3 do not support this assumption. The model shows a somewhat negative effect to no effect of MST on being in school at the age of 18, and the effect is nearly identical before (Table 1) and after (Table 3) using PSM.[i]

We also examined how the matching estimate was influenced by introducing fictive confounders in the model. These confounders were simulated and are shown in appendix 1. Overall, the results from the sensitivity analysis suggest that the baseline ATT is robust given unmeasured covariates similar to those included in our models.

4. Discussion

This study reports on school enrolment at the age of 18 among all youth who received MST at least once for severe behavioural and/or substance abuse problems in the 2002–2010 period (N = 1,086). We compared school enrolment for this MST group with enrolment for a comparable group of youth who received usual services (TAU) (N = 6,394).

Contrary to our assumptions, school enrolment was somewhat lower in the MST group than in the TAU group. Based on the descriptive statistics, which showed that the MST group was over-represented by characteristics that are related to positive school outcomes, we asked if the effect of MST on school enrolment is even smaller than we anticipated. We used a quasi-experimental design and PSM based on 17 important background characteristics to construct a comparison group (TAU) that could be considered randomly selected. The comparison based on PSM showed the difference in school enrolment between the MST and TAU groups to be identical to the difference revealed in the previous analysis.

These findings can be interpreted in two ways. First, the results indicate that MST is not able to utilize the resource potential that seem to be available in the group of youth who receive MST. Considering the MST inclusion/exclusion criteria, it is not surprising that the MST youth had somewhat different family background characteristics. Nevertheless, the results showed that the favourable family situations of the MST youth did not improve their school enrolment beyond that of the TAU youth. On the other hand, the difference in school enrolment between the MST and TAU youth did not change after adjusting for the somewhat more favourable family situations of the MST group relative to the TAU group. Surprisingly,

the results were nearly identical after using a quasi-experimental design based on PSM to compare the school enrolment of the two groups. These results indicate that school enrolment at age 18 of youth with severe behavioural and/or substance abuse problems is independent of the child welfare services treatment they receive (MST or the usual treatment).

Second, these results can also be interpreted to imply that the usual Norwegian child welfare services are high quality for this group of youth, which is argued by e.g. T. Ogden and C. A. Halliday-Boykins (2004). In their study, the results show that MST was more effective than usual child welfare services at reducing problematic behaviour and out-of-home placements. However, these positive outcomes were found to be more modest than previous results in US. According to Ogden and Halliday-Boykins this is to be explained by the differences in the usual child welfare services between the two countries. While the Norwegian system offers a broad array of social services and mental health treatment for e.g. juvenile offenders, usual services in US consists mainly of probation office visits with referral to social services if necessary (T. Ogden & C. Halliday-Boykins, 2004: 82). The results in the present study imply that usual services in Norway are successful in getting youths who engage in antisocial behaviour and or substance abuse back on track.

On the other hand, it could be argued that school attendance at the age of 18 is a poor measurement method of the effect on MST/TAU when it comes to this group of youth. Several RCT-studies of MST include a much more complex outcomes variable (e.g. delinquency, psychopathology, substance use, family functioning, peer relations, and out-of-home placements in addition to educational outcomes (van der Stouwe et al., 2014) which may contribute to a more solid measurement of the effect of MST. In addition, school attendance may be unattainable or not the right course for many of these youths. Consequently, using additional measures than school enrolment would be preferable. Thus, using school enrolment as the only measure of the effect of MST versus TAU is a limitation of this study. However, as previous mentioned, school enrolment is strongly correlated with favourable long-terms outcomes (De Ridder et al., 2012; Hammarström & Janlert, 2002; Rumberger & Lamb, 2003), and motivation to change current behaviour (Tighe et al., 2012). Even so, future research should include other outcome measures (especially information about employment/unemployment) in addition to health-related issues such as substance abuse and criminal behaviour. However, it should be noted that the outcome measure in this study comprised more than just the most successful level of educational progress, as we included all types of education (e.g., junior high school, bible schools or other schools without formal degrees).

5. Conclusion

Educational outcomes following MST were more or less identical with those following TAU, even though the group of youth who received MST came from families with somewhat more resources. We determined this by using a quasi-experimental design to analyse the data. These results provide additional support for previous conclusions and recommendations made by independent researchers outside the MST teams.

Acknowledgements

This study was funded by Norwegian Directorate for Children, Youth and Family Affairs. We thank our colleagues at NOVA for useful comments on earlier drafts, and the anonymous reviewers and the editor for providing extensive and valuable comments. We wish to confirm that there are no known conflicts of interest associated with this publication and there has been no significant financial support for this work that could have influenced its outcome.

References

Backe-Hansen, E., Madsen, C., Kristofersen, L. B., & Hvinden, B. (2014). Barnevern i Norge 1990–2010. En longitudinell studie [Child Welfare in Norway 1990–2010. A Longitudinal Study] (Vol. 9/14). Oslo: Nordic Social Research. Report number 9/14.

Barth, R. P., Greeson, J. K. P., Guo, S., Green, R. L., Hurley, S., & Sisson, J. (2007). Changes in family functioning and child behavior following intensive in-home therapy. *Children and Youth Service Review, 29*(8), 988–1009.

Blossfeld, P. N., Blossfeld, G. J., & Blossfeld, H.-P. (2015). Educational expansion and inequalities in educational opportunity: Long-term changes for East and West Germany. *European Sociological Review, 31*(2), 144–160.

Brown, T. L., Henggeler, S. W., Schoenwald, S. K., Brondino, M. J., & Pickrel, S. G. (1999). Multisystemic treatment of substance abusing and dependent juvenile delinquents: Effects on school attendance at posttreatment and 6-month follow-up. *Children's services: Social policy, research and practice, 2*(2), 81–93.

Christiansen, Ø. (2015). Hjelpetilak i barnevernet - en kunnskapsstatus [In-home services in the child welfare system - a review]. Bergen: Uni Research Helse, Regionalt kunnskapssenter for barn og unge (RKBU Vest).

Cook, T. D., & Campbell, D. T. (1979). *Quasi-experimentation: Design and analysis issues for field settings*. Chicago: Rand McNally.

De Ridder, K. A. A., Pape, K., Johnsen, R., Westin, S., Holmen, T. L., & Bjørngaard, J. H. (2012). School dropout: a major public health challenge: a 10-year prospective study on medical and non-medical social insurance benefits in young adulthood, the Young-HUNT 1 Study (Norway). *Journal of Epidemiology and Community Health, 66*(11), 995–1000. doi: 10.1136/jech-2011-200047

Gail, M. H., & Benichhou, J. (Eds.). (2000). *Encyclopedia of Epidemiologic Methods*. Chichester: John Wiley & Sons Ltd.

Hammarström, A., & Janlert, U. (2002). Early unemployment can contribute to adult health problems: results from a longitudinal study of school leavers. *Journal of Epidemiology and Community Health, 56*(8), 624–630. doi: 10.1136/jech.56.8.624

Henggeler, S. W., Rowland, M. D., Randall, J., Ward, D. M., Pickrel, S. G., Cunningham, P. B., Miller, S. L., Edwards, J., Zealberg, J. J., Hand, L. D., & Santos, A. B. (1999). Home-based multisystemic therapy as an alternative to the hospitalization of youths in psychiatric crisis: Clinical outcomes. *Journal of the American Academy of Child and Adolescent, 38*(11), 1331–1339.

Henggeler, S. W., Schoenwald, S. K., Swenson, C. C., & Borduin, C. M. (2006). Methodological critique and meta-analysis as Trojan horse. *Children and Youth Services Review, 28*(4), 447–457.

Ichino, A., Meali, F., & Nannicini, T. (2008). From temporary help jobs to permanent employment: What can we learn from matching estimators and their sensitivity? *Journal of Applied Econometrics, 23*(3), 305–327.

Jackson, M. (2013). *Determined to Succeed. Performances versus Choice in Educational Attainment*. Standford, California: Stanford University Press.

Jonsson, J. O., & Rudolphi, F. (2011). Weak performance - strong determination: School achievement and educational choice among children of immigrants of Sweden. *European Sociological Review, 27*(4), 487–508. doi: 10.1093/esr/jcq021

Kravitz, R. L., Duan, N., & Braslow, J. (2004). Evidence-based medicine, heterogeneity of treatment effects, and the trouble with averages. *Milbank Quarterley, 82*(4), 661–687.

Littell, J. H. (2006). The case for multisystemic therapy: Evidence or orthodoxy? *Children and Youth Services Review, 28*(4), 458–472.

Littell, J. H. (2008). Evidence-based or biased? The quality of published reviews of evidence-based practices. *Children and Youth Services Review, 30*(11), 1299–1317.

Littell, J. H., Campbell, M., Green, S., & Toews, B. (2005). Multisystemic therapy for social, emotional, and behavioral problems in youth aged 10–17. *Cochrane Database of Systematic Reviews, 2005*(4), Art. No.: CD004797.

Luo, Z., Gardiner, J. C., & Bradley, C. J. (2010). Applying Propensity Score Methods in Medical Research: Pitfalls and Prospects. *Med Care Res Rew, 67*(5), 528–554.

Marti, N., Stice, E., & Springer, D. W. (2010). Substance use and abuse trajectories across adolescence: A latent trajectory analysis of a community-recruited sample of girls. *Journal of adolescence, 33*(3), 449–461.

Mensch, B. S., & Kandel, D. B. (1988). Dropping out of high school and drug involvement. *Sociology of Education, 61*(April), 95–113.

MST Services Inc. (2015). Retrieved 4 March, 2015, from http://mstservices.com/index.php

Nannicini, T. (2007). Simulation-based sensitivity analysis for matching estimators. *The Stata Journal, 7*(3), 334–350.

Ogden, T., & Hagen, K. A. (2006). Mulitisystemic treatment of serious behaviour problems in youth: Sustainability of effectiveness two years after intake. *Child and Adolescent Mental Health, 11*(3), 142–149.

Ogden, T., Hagen, K. A., & Andersen, O. (2007). Sustainability of the effectiveness of a programme of multisystemic treatment (MST) across participant groups in the second year of operation. *Journal of Children's Services, 2*(3), 4–14.

Ogden, T., & Halliday-Boykins, C. (2004). Mulitsystemic treatment of antisocial adolescents in Norway: Replication of clinical outcomes outside of the US. *Child and Adolescent Mental Health, 9*(2), 77–83.

Ogden, T., & Halliday-Boykins, C. A. (2004). Multisystemic treatment of antisocial adkolescents in Norway: Replication of clinical outcomes outside of the US. *Child and Adolescent Mental Health, 9*(2), 77–83.

Olsson, T. (2010). Intervening in youth problem behavior in Sweden: a pragmatic cost analysis of MST from a randomized trial with conduct disordered youth. *International Journal of Social Welfare, 19*(2), 194–205.

Patterson, G. R., DeBaryshe, B. D., & Ramsey, E. (1989). A developmental perspective on antisocial behavior. *American Psychologist, 44*(2), 329–335.

Pettersen, S. V., & Østby, L. (2013). Immigrants in Norway, Sweden and Denmark. *Samfunnsspeilet [Statistics Norway's journal], 27*(5), 20–26.

Rosenbaum, P. R. (2002). Covariance Adjustment in Randomized Experiments and Observational Studies. *Statistical Sciences, 17*(3), 286–327.

Rosenbaum, P. R., & Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrica, 70*(1), 41–55.

Rumberger, R. W., & Lamb, S. P. (2003). The early employment and further education experiences of high school dropouts: a comparative study of the United States and Australia. *Economics of Education Review, 22*(4), 353-366. doi: 10.1016/S0272-7757(02)00038-9

Shadish, W. R. (2013). Propensity score analysis: promise, reality and irrational exuberance. *Journal of Experimental Criminology, 9*(2), 129–144.

Statistics Norway. (2014, 07.11.2014). Facts about education in Norway 2012. Key figures 2012. from http://www.ssb.no/en/utdanning/artikler-og-publikasjoner/facts-about-education-in-norway-2014

Tighe, A., Pistrang, N., Casdagli, L., Baruch, G., & Butler, S. (2012). Multisystemic therapy for young offenders: Families experiences of therapeutic processes and outcomes. *Journal of Family Psychology, 26*(2), 187–197.

Timmons-Mitchell, J., Bender, M. B., Kishna, M. A., & Mitchell, C. C. (2006). An independent effectiveness trial of multisystemic therapy with juvenile justice youth. *Journal of Clinical Child and Adolescent Psychology, 35*(2), 227–236.

van der Stouwe, T., Asscher, J. J., Stams, G. J. J., Dekovic, M., & van der Laan, P. H. (2014). The effectiveness of multisystemic therapy (MST): A meta-analysis. *Clinical Psychology Review, 34*(6), 468–481.

Weiss, B., Han, S., Harris, V., Catron, T., Caron, A., Gallop, R., & Guth, C. (2013). An independent randomized clinical trial of multisystemic Therapy with non-court-referred adolescents with serious conduct problems. *Journal of Consulting and Clinical Psychology, 81*(6), 1027–1039.

Table 1

Descriptive statistics for youth with behaviour problems and/or substance abuse problems (per cent)

	MST		TAU		*p*	
	#	%	#	%		
In school at the age of 18	646	59,5	4 022	62,9	0,032	*
Characteristics of the youth						
Gender						
Boys	600	55,2	3 948	61,7	0,000	**
Immigrant background						
Non-western immigrant background	61	5,6	847	13,2	0,004	**
Initial age for receiving child welfare services						
4 years old or younger	87	7,5	822	13,8	0,000	**
5 thru 9 years old	87	8,0	997	16,0	0,000	**
10 thru 14 years old	311	33,2	1 802	35,8	0,103	NS
15 thru 17 years old	444	51,3	1 928	34,5	0,000	**
Prior report of maltreatment	95	8,7	863	13,5	0,000	**
Year of birth						
1990	235	21,6	1 285	20,1	0,243	NS
1991	239	22,0	1 362	21,3	0,600	NS
1992	217	20,0	1 469	23,0	0,029	*
1993	234	21,6	1 260	19,7	0,161	NS
1994	161	14,8	1 018	15,9	0,359	NS
Average number of initiatives per year						
0	779	71,7	3 433	53,7	0,000	**
1	208	19,2	1 965	30,7	0,000	**
2	74	6,8	791	12,4	0,000	**
3	19	1,8	170	2,7	0,078	NS
4 or more	6	0,6	35	0,6	0,983	NS
Characteristics of the parents						
Educational level						
Below upper secondary education	211	19,4	1 784	27,9	0,000	**
Upper secondary education	525	48,3	3 233	50,6	0,176	NS
Higher education	350	32,2	1 377	21,5	0,000	**
Both parents with non-western background	61	5,6	847	13,3	0,000	**
Income						
NOK 149 999 or less	129	11,9	1 133	17,7	0,000	**
NOK 150 000 thru 249 999	455	41,9	3 269	51,1	0,000	**
NOK 250 000 thru 349 999	391	36,0	1 613	25,2	0,000	**
NOK 350 000 thru 449 999	88	8,1	278	4,3	0,000	**
NOK 450 000 thru 549 999	12	1,1	51	0,8	0,306	NS
NOK 550 000 or more	11	1,0	50	0,8	0,434	NS
Unemployed parent	15	1,4	54	0,8	0,004	**
Family received social welfare						

No social welfare	766	70,5	4 247	66,4	0,000	**
Less than one year	116	10,7	661	10,3	0,002	**
More than one year	204	18,8	1 486	23,2	0,000	**
Parents married	428	39,40	1 901	29,70	0,077	NS
Mother's age at birth						
19 years old or younger	90	8,3	523	8,2	0,905	NS
20 thru 29 years old	691	63,6	4 087	63,9	0,853	NS
30 thru 39 years old	284	26,2	1 668	26,1	0,965	NS
40 years old or older	21	1,9	116	1,8	0,786	NS
Father's age at birth						
19 years old or younger	17	1,6	110	1,7	0,715	NS
20 thru 29 years old	584	53,8	3 164	49,5	0,009	**
30 thru 39 years old	393	36,2	2 504	39,2	0,063	NS
40 years old or older	92	8,5	616	9,6	0,226	NS
Substance abuse by primary care giver	32	2,9	250	3,9	0,007	**
Geographical charachteristics						
Residential region of Norway						
Northern Norway	181	16,7	796	12,4	0,000	**
Mid-Norway	98	9,0	574	9,0	0,960	NS
Western Norway	284	26,2	1 696	26,5	0,796	NS
Eastern Norway	422	38,9	2 965	46,4	0,000	**
Southern Norway	101	9,3	363	5,7	0,000	**
Population size (living in municipalities with less than 5000 inhabitants)	101	9,3	856	13,4	0,000	**
N	**1 086**	**100,0**	**6 394**	**100,0**		

Note: * $p < .05$, ** $p < .01$, NS =not statistical significant

Table 2

Effect on program participation for baseline variables, and mean values before and after matching. N=7 478.

	Independent variables	Reference	Effect on program participation, odds ratio (95% confidence interval)	Sample status	Mean	
					Treated (MST)	Control (TAU)
Characteristics of the youth	**Gender**	**girl**	**1.31 (1.15 - 1.49)**	Unmatched	1,448	1,383
				Matched	1,447	1,453
	Year of birth	**1990**	**0.98 (0.93 - 1.03)**	Unmatched	1991,9	1991,9
				Matched	1991,9	1991,9
	Born in Norway	**no**	**1.36 (1.10 - 1.69)**	Unmatched	0,901	0,869
				Matched	0,901	0,901
	Debut age in the Child Welfare Services	**4 yrs or younger**	**1.51 (1.40 - 1.62)**	Unmatched	3,284	2,909
				Matched	3,282	3,284
	Prior report of maltreatment	**no**	**0.52 (0.42 - 0.65)**	Unmatched	0,088	0,158
				Matched	0,089	0,093
	Annual average number of measures from the CWS	**0**	**0.64 (0.58 - 0.70)**	Unmatched	0,402	0,656
				Matched	0,403	0,412
Characteristics of the parents	**Mothers age**	**19 yrs or younger**	**1.01 (0.90 - 1.12)**	Unmatched	3,217	3,215
				Matched	3,217	3,219
	Fathers age	**19 yrs or younger**	**0.89 (0.81 - 0.98)**	Unmatched	3,516	3,567
				Matched	3,514	3,520
	Parental educational level	**below upper secondary**	**1.48 (1.35 - 1.62)**	Unmatched	2,128	1,936
				Matched	2,126	2,126
	Both parents with non-western background	**no**	**0.39 (0.30 - 0.51)**	Unmatched	0,056	0,132
				Matched	0,056	0,059
	Parents marital status	**not married**	**1.12 (0.99 - 1.28)**	Unmatched	0,483	0,454
				Matched	0,482	0,479

	Family received social welfare benefit	**no**	**0.67 (0.62 - 0.73)**	Unmatched	1,523	1,795
				Matched	1,524	1,524
	Family income	**NOK 149 999 or less**	**1.36 (1.27 - 1.46)**	Unmatched	2,477	2,217
				Matched	2,474	2,491
	Parental unemployment	**no**	**2.09 (1.25 - 3.49)**	Unmatched	0,018	0,009
				Matched	0,017	0,014
	Substance abuse by primary caregiver	**no**	**0.57 (0.37 - 0.86)**	Unmatched	0,023	0,040
				Matched	0,023	0,025
Geographical charachteristics	**Region of living=Northern Norway**	**no**	**1.41 (1.18 - 1.68)**	Unmatched	0,167	0,124
				Matched	0,167	0,133
	Region of living=Mid-Norway	**no**	**1.01 (0.80 - 1.26)**	Unmatched	0,090	0,090
				Matched	0,090	0,095
	Region of living=Western Norway	**no**	**0.98 (0.85 - 1.14)**	Unmatched	0,262	0,265
				Matched	0,260	0,284
	Region of living=Eastern Norway	**no**	**0.73 (0.64 - 0.84)**	Unmatched	0,389	0,464
				Matched	0,389	0,438
	Region of living=Southern Norway	**no**	**1.70 (1.35 - 2.15)**	Unmatched	0,093	0,057
				Matched	0,093	0,050
	Living in a municipality with a population less than 5 000	**no**	**0.49 (0.39 - 0.61)**	Unmatched	0,086	0,162
				Matched	0,086	0,089

Abbreviations: OR= Odds ratio, CI= Confidence intervals.

Table 3

Effects of MST on being in school at the age of 18 (n = 7,478) for youth with behaviour problems and/or substance abuse problems.

In school at the age of 18, estimations by	Value if MST	Value if TAU	Mean bias (%)	Risk difference	T-value	p-value	
Nearest neighbour with replacement	0,595	0,605	2,9	-0,010	-0,44	0,660	
Nearest neighbour without replacement	0,595	0,618	2,5	-0,023	-1,10	0,271	
Nearest five neighbours	0,595	0,637	1,6	-0,042	-2,35	0,019	*
Caliper with replacement	0,595	0,605	2,9	-0,010	-0,44	0,660	
Caliper without replacement	0,595	0,618	2,5	-0,023	-1,10	0,271	
Radius matching, caliper 0.017	0,595	0,632	0,9	-0,037	-2,28	0,023	*
Kernel	0,595	0,631	2,0	-0,036	-2,20	0,028	*

Note: The caliper (distance to nearest control) is set to 0.25 times the standard error of the propensity score.

*$p < .05$.

Appendix

Overall, we found no strong support for an increased likelihood of being in school at the age of 18 given MST treatment (when compared to matched controls with TAU). The risk difference ranged from -0.010 thru -0.042. This approach could in principle correct for bias given that all relevant variables were observed and measured without error. However, when a potential confounder to the treatment is unobserved, this approach can only correct for bias to the extent that the unobserved confounder(s) are correlated with the observed covariates.[1] Thus, we cannot exclude the possibility that the treatment assignment is potentially entangled with the outcome due to unobserved characteristics than the ones that have been adjusted for (e.g. administrator introduced bias upon recruitment to MST, differences in IQ, school grades etc.).

The table (Appendix 1) reports the radius-based treatment effects obtained with and without a simulated confounder using sensatt in STATA 13. We specified the model using the radius matching method. The sensitivity analysis functions as a way to specify how a potential unmeasured confounder could affect the ATT. The table shows how the unmeasured confounder (*U*) would be present given different combinations of treatment and outcome status: the first number denotes the treatment status (1/0) and the second denotes the outcome status (1/0). For instance, an unmeasured confounder equal to "family received social welfare benefit" would be expected to be more correlated with

- the control group ($p0$) than with the treatment group ($p1$).
- individuals without the outcome ($p10$ and $p00$) than those whom were in education at the age of 18 ($p11$ and $p01$).

Appendix 1 shows five different models in addition to the baseline model without any confounder. The neutral confounder shows that given an unmeasured confounder with equal probability for the four combinations of treatment and outcome, we would expect no influence on the selection and outcome effects, and thus no influence on the estimation of the treatment effect. The next three models shows three simulated confounders calibrated to resemble known covariates at baseline. These models indicate somewhat larger influences on both the selection and outcome effects. Including such confounders had modest if any influence on the estimation of the treatment effect. In addition we also simulated a strong

[1] Luo, Z., Gardiner, JC., Bradley, CJ. (2010) Applying Propensity Score Methods in Medical Research: Pitfalls and Prospects. Med Care Res Rev, 67(5), 528-554.

confounder. This confounder was unlike any of the observed covariates, and the treatment group ($p1$) had a very high probability of receiving this compared to the control group ($p0$). Furthermore, individuals without both treatment and outcome ($p00$) had a higher probability of this confounder compared to those without treatment but with the outcome ($p01$). This confounder had a large influence on both the selection and the outcome effect, and resulted in a treatment effect close to zero. The presence of such an unmeasured confounder is less likely since such a confounder would need to explain almost the entire baseline estimate of ATT. Overall; we conclude that the results from the sensitivity-analysis suggest that the baseline ATT estimates are robust.

Appendix 1

Sensitivity analysis for propensity score matching estimators with respect to the treatment effects of MST on being in school at the age of 18.

	Confounder *U* = 1 by treatment/outcome				Outcome effect (OR)	Selection effect (OR)	ATT	SE
	***p*11**	***p*10**	***p*01**	***p*00**				
No confounder	0,00	0,00	0,00	0,00			-0,037	0,016
Neutral confounder	0,50	0,50	0,50	0,50	1,00	1,00	-0,037	0,016
Confounder like							0,000	0,000
Family received social welfare benefit	0,33	0,37	0,49	0,52	0,89	0,53	-0,038	0,016
Parental educational level	0,82	0,78	0,74	0,76	1,28	1,60	-0,034	0,016
Both parents with non-western background	0,06	0,05	0,14	0,12	1,17	0,36	-0,030	1,016
Strong confounder	0,76	0,76	0,10	0,60	0,07	8,16	-0,003	0,017

Note: *U*=unmeasured confounder, *p*11=probability of *U* given treatment==1/outcome==1, *p*10=probability of *U* given treatment==1/outcome==0, *p*01=probability of *U* given treatment==0/outcome==1, *p*00=probability of *U* given treatment==0/outcome==0, OR= Odds ratio, ATT=average treatment on the treated, SE=standard error.

- School enrolment among youths with antisocial behaviour or substance abuse
- The effectiveness of multisystemic treatment (MST) and treatment as usual (TAU)
- Results from registry data using Propensity Score Matching (PSM)

[i] We used imputed values for cases with missing covariate information at baseline. The missing values were assumed to be MCAR since we did not have any theoretical assumptions stating otherwise. However, we tested if these cases had any influence on the results by removing them from the model. The results did not deviate from the full model with imputed values.